cultural Department, Washington, March 16th, and the St. Louis Academy, March 17th, 1863.

Donations for the Library were received from Mr. Edward Miller, the Bureau of Mines at Paris, the London. Meteorological Society, Royal Geographical Society, and Society of Arts, the Scottish Antiquarian Society, the Laval University at Quebec, Mr. Henry Hall, of Rutland, Vermont, the New Bedford Free Public Library, Silliman's Journal, the Franklin Institute, the Pennsylvania Historical Society, and the Cincinnati Young Men's Mercantile Library Association.

Professor Lesley read the following communication from President J. W. Dawson, of McGill College, Montreal.

Note on Mr. Lesley's Paper on the Coal-measures of Cape Breton.

The new facts and general considerations on the Nova Scotia coal-field contained in this paper, are of the highest interest to all who have worked at the geology of Nova Scotia. I think it my duty, however, to take exception to some of the statements, which I think a larger collection of facts, would have induced Mr. Lesley himself to modify. My objections may be stated under the following heads.

(1.) It is scarcely safe to institute minute comparisons between the enormously developed coal-measures of Nova Scotia, and the thinner contemporary deposits of the West, any more than it would be to compare the great marine limestones of the period at the West, with the slender representatives of the part of the group to the eastward.

(2.) There is the best evidence that the coal-measures of Nova Scotia never mantled over the Devonian and Silurian hills of the Province, but were on the contrary, deposited in more or less separate areas on their sides.

(3.) Any one who has carefully compared the coal-measures of the Joggins with those of Wallace and Pictou, must be convinced of the hopelessness of comparing individual beds, even at this comparatively small distance. *A fortiori* detailed comparisons with Pennsylvania and more distant localities must fail.

(4.) I do not think that any previous observer has supposed that the coal-measures of Eastern Cape Breton represent the whole of the

coal formation of Nova Scotia. The "Upper coal-measures" of my papers on Nova Scotia are certainly wanting, and probably the Sydney coal-field exhibits no beds higher than the middle of No. 4 of Logan's Joggin section.

(5.) The whole of the coal-beds in the Joggin section belong to the *Upper* and *Middle* coal-measures. It is quite incorrect to identify No. 6 of Logan's section with the *Lower* coal-measures. These do not occur at the Joggins, but are found in Nova Scotia, as in Virginia and Southern Pennsylvania, at the base of the system, under the marine limestones. The Albert beds are the equivalents of these Lower measures, and not of the Pictou coal. In my paper on the Lower Carboniferous coal-measures (Journal of Geological Society of London, 1858), will be found a summary of the structure of the Lower coal-measures, as shown at Horton Bluff, and elsewhere. The term "true coal-measures," quoted by Mr. Lesley, does not mean in my description, the Middle coal-measures, but merely that part of them holding the workable coal-seams.

(6.) Whatever may be the value of M. Lesquereux's applications of the fossil flora to the identification of coal-seams in the West, I am prepared to state, as the result of an extensive series of observations, still for the most part unpublished, that in Nova Scotia, the flora is identical throughout the whole enormous thickness of the Middle coal-measures, and that the differences observable between different seams, are attributable rather to difference of station and conditions of preservation, than to lapse of time. It is, indeed, true, as I have elsewhere explained, that the assemblages of species in the Lower, Middle, and Upper coal-measures, may be distinguished; but within these groups the differences are purely local, and afford no means for the identification of beds in distant places.

(7.) I do not desire to offer any opinion on the questions raised by some American geologists, as to the extension of the term carboniferous to the Chemung group; but I know as certain facts, that the flora of the Lower coal-measures, under the marine limestones and gypsums of Nova Scotia, is wholly carboniferous, and that the *flora*, on which alone I consider myself competent to decide, of the Chemung of New York, as now understood by Professor Hall, and others, and also of the groups in Pennsylvania, named by Rogers, Vergent, and Ponent (? IX and X of Mr. Lesley), is as decidedly Devonian, and quite distinct from that of the carboniferous period.*

* See Paper on Devonian Flora of Eastern America, Jour. Lond. Geol. Soc. November, 1862.

For Mr. Lesley's ability as a stratigraphical geologist, I have the highest respect; and with reference to the present subject, would merely desire to point out that he may not have possessed a sufficient number of facts to warrant some of his generalizations, on which in the meantime I would, for the reasons above stated, desire geologists to suspend their judgment.

J. .W DAWSON.

McGILL COLLEGE, MONTREAL,
February 18th, 1863.

Mr. Lesley remarked that he read this communication of his friend, Professor Dawson, with great pleasure, as it would prevent any mistake about the nature and importance of the discussion, and any undue weight being attached to his own suggestions; that no one was more convinced than himself that there could be no excuse for dogmatism where so little was known, and therefore, that he had intended rather to suggest than to defend those opinions expressed in his paper, which had drawn down so earnest and valuable a caveat from so high a source. To defend them would require long and systematic researches on the ground, if even then, the too easily accepted present standpoint of palæontology would not hide the truth from view behind immovable obstacles. So long as apparent specific identity in organic forms continues to be accepted as the supreme test of stratigraphical horizon, discord is inevitable. When palæontology is prepared to return under the mild dominion of her mother, lithology, which she has at least one-half repudiated, geology will advance more rapidly in her work.

Professor Dawson's first objection is a begging of the very question, Whether the coal-measures of Nova Scotia *are* "enormously developed." That, in one little spot of the earth's surface like Nova Scotia, and that too midway between the great coal areas of America and those of Europe, wherein the thickness of coal-measures proper range from 2000 to 5000 feet, if they even attain the latter size, there should be an anomalous deposit of 25,000 feet, is incredible. What the great Bohemian palæontologist, by unerring instinct, said to us after our thirty years' war over the Taconic system, *there must be a mistake somewhere*, I must repeat to those who so "enormously develop" the Nova Scotia coal-measures. And my inten-

tion in the paper on Nova Scotia coal was only to suggest one formula on which the error might be discussed. I distinctly repudiated the safety of instituting "minute comparisons." My comparison of the Cape Breton coals and the column at Pittsburg, was carefully made in the most general manner, and the resemblance called a coïncidence. But the value of the comparison remains; for it affords a new argument in favor of the *family likeness* of those parts of the general coal-measures of different countries, which have a right to the specific title of "productive coals." The argument also remains good, that if 2000 feet of coal-measures in Missouri can be recognized in 2000 feet of coal-measures in Kentucky, Virginia, and Eastern Pennsylvania, the very same system of beds, bed for bed, being demonstrated first by stratigraphy, and then by palæontology (and such is the fact), why not in Nova Scotia? Even granting (3) that sufficient skill and care and opportunity combined have hitherto failed to identify the coals of the Joggins with those of Wallace and Pictou, there is still hope at the bottom of the box. Before Lesquereux perched himself like a Simon Stylites on the slack heap at the mine's mouth, our own identification of individual beds was very imperfect, and the search for a complete system of identification had been abandoned with the same sense of hopelessness. But how is it now? There certainly may be special difficulties in Nova Scotia; there are such at Pottsville; in Michigan; but they are exceptions which prove the rule, instead of affording an *a fortiori* argument against it.

I have no doubt that some of the coal-measures of the British Provinces may have been "deposited in more or less separated areas on the sides of the Devonian and Silurian hills," as Professor Dawson says (2). But I confess to a complete scepticism of the great extent which has been assigned to this nonconformability of the coal-measures upon the Lower Rocks; first, because most of the Island of Cape Breton, and much of the surface of Nova Scotia and New Brunswick are confessedly unstudied and almost unknown; secondly, because the incredible thickness assigned to the coal-measures, throws doubt upon the positions assigned to the nonconformable horizons; thirdly, because the coal-beds themselves stand almost vertical in many places round the shores; fourthly, because the mountains of Nova Scotia, with apparently conformable carboniferous limestones, have apparently an Appalachian structure and aspect, have suffered vast denudation, exhibit cliff outcrops and section ravines, and may just as well have carried coal upon their original backs, as we can prove that our Tussey, Black Log, Nescopec, Mahoning, Buffalo, Tuscarora,

Brush, and other Silurian and Devonian mountains did. There is an immense nonconformable chasm in the column west of the Hudson River, and the Catskill Mountains over it have no coal upon their backs; but the coal comes in regularly enough on them at the Lehigh, (a less distance than from Sydney to St. Peters, or from Pictou to Windsor), and the nonconformability in the Upper Silurian and Devonian has already disappeared.

Professor Dawson's fourth objection would be good, if I had really "supposed the coal-measures of Eastern Cape Breton to represent the whole of the coal-measures of Nova Scotia." But I only suggested that they may prove to be the equivalents of the system of *productive coal-measures;* that is all. Between the Monongahela and the Ohio, our column of productive coals is capped by another of barren shales and soft sandstones of unknown height, by one estimate 3000 feet thick; and part of this column may represent the so-called Permian measures, which, in Kansas, cap conformably the coal-measures. Having no knowledge of the fossils, I have no desire to oppose the conclusions of Professor Dawson, as to the part of the column of the Joggins to which the Glace Bay coals apply, but hope that his accurate handling of them will secure some certainty about it. It was the grouping of the beds, and not the fossils, which I wished to bring into prominent notice; because the doctrine of isolated basins, when unfounded or overapplied, is as injurious to lithological truth, as the careless identification of surface aspect may at any moment prove to palæontology. I willingly leave to accomplished palæontologists like Professor Dawson, the discussion of the grand generalization embodied in his sixth objection; but I may be permitted to believe that it has had its birth in the doctrine of isolated basins, and that the two must stand or fall together. It also seems to me to involve radical inconsistencies; for if I comprehend it, it asserts, 1. That the flora of the whole coal-measures (25,000 feet?) is identical; that is, the vertical distribution of each and all the plants is complete from the bottom to the top. 2. That nevertheless, there are differences observable between different coal-beds. 3. That these are attributable rather to difference of station and conditions of preservation, than to lapse of time; that is, if we could take the beds, each one in its whole extent and its fossils in their original condition, there would be no differences observable between different seams after all. 4. That groups or assemblages of species in the Lower, Middle, and Upper coal-measures may nevertheless be distinguished; that is, while each and every species may be found occasionally in all parts of the

column from bottom to top, yet this happens in such a manner as to group some of them more abundantly, or in certain peculiar proportions in the Lower, others in the Middle, and others in the Upper portions of it. 5. That, after all, however, these groups are not persistent, but differ at different localities, and are as worthless as the specific forms themselves for the identification of a single bed in more than one place.—Is it possible that all this has been made out, or *can* be made out, except in a country of *horizontal* coal-measures, well opened for study, where the stratification can be established beforehand, and the range of the fossils be doubtless?

In conclusion I would say, that the want of clearly defined and applied names is a drawback to such a discussion. The discussion is in fact *initially* one of names, viz., how far down the name Carboniferous must be carried; what are the Lower coal-measures, &c. But *in the end*, it is a question of vital importance to the value of the palæontological *imprimatur* upon stratigraphical and structural deductions from field work. Is the discovery of specific forms to keep all our geological *niveaux* in a perpetual mirage-flicker? Are we never to know from day to day, whether we are at work in Devonian or Carboniferous, in Trias, (Dyas,) or Lias? Why not at once obey the marriage law of the weaker sex, and give up our names for our lord's? Let geology forget the virgin nomenclature of her youth, and rewrite her books with such titles for her chapters as these: "The Spirifeferous Formation; The Lepidodendriferous Formation; The Lower Thecodont; The Middle Baculite; The Upper Pterodactylian Formations." Why has this not already been done? Simply because it cannot be done. No palæontologist has yet been bold enough even to propose it. Yet as I believe, the 25,000 feet of coal-measures in the British Provinces, will be found to be one of the many unconscious *realizations* of this idea, when no one can be found to *nominate* it openly. The whole palæozoic system at its thickest place in Southeast Pennsylvania and Middle Virginia, is but 35,000 feet. It is not unreasonable then to *suggest*, if not to affirm, that the vast column of so-called coal-measures in Nova Scotia will take in all that part of the palæozoic column which has furnished coal, and that is from the top downwards nearly to the Upper Silurian, as Plate II will show.

A letter was received from Dr. C. M. Wetherill, containing some notice of his observations on the deterioration of ether from age, and its absorption of fusil oil in the special instance described.

DEPARTMENT OF AGRICULTURE,
WASHINGTON, D. C., March 16th, 1863.

TO SEC. AM. PHIL. SOC., PHILADA.

DEAR SIR: I have lately made an observation in my laboratory, which I desire to have recorded, as throwing light upon the deterioration of ether by age. I have communicated it to Dr. W. G. T. Morton, who deems it very interesting in its relation to anæsthesia. The subject is worthy of a further examination, which my official duties at present forbid.

I brought with me from Ohio a quart bottle of ether, half of which had been used in the course of former chemical investigations, and which had been found pure. The bottle was stopped with cork, *through which the evaporation was too slow to be perceptible.* About three months ago, this bottle, by a mistake of measurement by the carpenter in the glass case provided for my chemicals, came in juxtaposition with three quart bottles of pure fusil oil. The latter was contained in glass stoppered bottles, the stoppers covered with bladder. One of these bottles had been opened to demonstrate the properties of fusil oil in connection with spirits, and was replaced without restoring the bladder. After this the odor of fusil oil was very perceptible in the neighborhood of the case. About three months later, the ether bottle was taken, and a portion of its contents used for determining the amount of fusil oil in wines and brandies, when it was at once discovered that the ether itself contained fusil oil. On permitting the spontaneous evaporation of the ether in a watch-glass, the oil was observed in abundant *globules*, the odor was that of amylic alcohol, and the irritating action upon the cuticle of the nose accidentally touching it was very strong. Very pressing official duties prevented a chemical examination by analysis of the oil absorbed by the ether, but the odor was alone sufficient to trace it to the fusil oil bottles. The ether had not been employed until used for the wine and brandy experiments, and had certainly not been meddled with by anybody. The only explanation I can give of the phenomena is, that the bottle of ether standing in an atmosphere of amylic alcohol vapor, had received the latter through the

pores of the cork, according to the laws of the diffusion of gases. If this view of the matter be correct, I can readily imagine how a bottle of ether might come into a position to absorb substances which might prove very injurious in cases of anæsthesia.

Yours, very respectfully,

CHARLES M. WETHERILL,

Chemist, Department of Agriculture.

Mr. Chase resumed the discussion of similar forms and meanings between the Chinese characters and the classical alphabets. While admitting that some of the resemblances might be accidental, he could not believe that they were all so. The pointings in a uniform direction are so numerous, that if the attention of scholars who are able to study the Chinese movements on their own soil could be fully aroused, important results might reasonably be looked for. The general character of these pointings was illustrated by the following remarks.

a. Most of the Chinese syllables that end in the sound of *i* (English ē), are pronounced *i* by some of the natives, and *ai* (English ī) by others, thus indicating the possible provincialism, that established the peculiar sound of the letter i in our language. One of the Chinese characters that represent this sound, resembles a small e in its ordinary form, while in the running hand it has the two forms of our written I and E. [See Plate I, fig. 1.]

b. Some of the Chinese hieroglyphs have both the form and the phonetic value of the modern script. Thus the radical for tooth, Ya, has the form of Y, Tsze, of Z (German *tseth*); Shan, of Hebrew *shin*, and Russian *sha* ; Fow, of German *fow* ; &c. [Pl. I, fig. 2.]

c. Not only are isolated letters found in Chinese, but also combinations of letters in syllables that retain a pronunciation similar to that indicated by the phonetic value of the letters. For example, the syllable *ki* (Eng. kē, or kī), is sometimes written in the following ways, to indicate three different meanings; *GE* or *GJ*, 𝔎𝔈 or 𝔎𝔍, 𝔎𝔑, [Pl. I, fig. 3], and each of these forms can be readily traced to the primitive radicals of which it is composed. The resemblance of the last form to the German 𝔎𝔑, as well as the retention of Chinese names for German letters, is suggestive of the resembclane that exists in the angular character of the Chinese and German texts.

d. The phonetic values of the primitive hieroglyphs, are sometimes apparently retained through a succession of different forms. The Chinese radical Tu, for example, which denotes "the earth," may be analyzed into two simple radicals, one of which corresponds to an ancient form of T, and the other is De Guignes' supposed representative for U, which resembles one of the Egyptian hieroglyphs for O and U. [Pl. I, fig. 13.] This radical is sometimes written in the form of T placed in U, and sometimes like a t connected with a V in such a way as to make an Arabic figure 2. [Pl. I, fig. 4.] Moreover, the root *tu*, in Chinese as well as in Sanscrit, embraced the idea of division among its meanings.

e. Some of the radicals are represented by two or more different forms that are found in two or more different alphabets. Thus the "mouth" radical is sometimes written in the form of Roman ᗜ, sometimes of the Greek *Δ*. The word *pi*, "to assist," is written with two Greek *π*'s, accompanied by a Roman P. [Pl. I, fig. 5.]

f. In most alphabets, forms nearly identical are employed to represent different letters, as in English E F; d b; p q; n u; Roman P, Greek *P*, and Hebrew ק. Many of these resemblances are found in Chinese, and a reference to the original hieroglyphs often suggests a plausible explanation of the resemblances.

g. If the fertility of resemblance between the Chinese and other alphabetic forms, is often confusing and puzzling, it is no more so than the similar fertility in systems that are purely alphabetic. Such instances as the employment of X to represent the sounds of T, Ch, and X in the Phenician, Greek, and Roman alphabets respectively, —of P for the sounds of P, Ts, Q, and R, &c.,—are so numerous that there is probably not a single alphabetic form that has not been appropriated at different times to several different letters, and there is not a single letter that does not present in its various forms, analogies to nearly half the other letters of the alphabet.

h. These resemblances are sometimes readily accounted for by their phonetic analogies. Thus L and R, B and P, C and G, sound so nearly alike, and are so often confounded, that one would naturally expect them to be represented by similar forms. But there are some curious instances of remoter affinity. For example, among the oldest Phenician inscriptions, the outline of something like a stone hatchet, is used to represent both D and R. These two sounds are to this day confounded by some of our Indian tribes. Most of the Dakotahs are unable to sound either L or R, and they invariably substitute D for each of those letters. The Chinese Taou, a knife,

with an outline resembling the Hebrew ה, seems naturally connected, both by its form and phonetic value, with the Phenician hatchet-shaped D and R.

i. The use of similar symbols for different radicals, seems sometimes to point to a still older primitive. In this way the supposed Egyptian equivalent for the ש, is connected with the Chinese symbols for Mountain, Mouth, Tooth, and Hand, perhaps through the intermediate idea of piercing, or projecting. [Pl. I, fig. 6. The first symbol is Egyptian, the others Chinese. The third form is employed by the Chinese, both for "mountain," and for "mouth."]

k. An apparent association with other supposed hieroglyphics, may be indirectly traced in some Chinese compounds, when such a connection would hardly be suspected in the simple elements. Thus the letter *G* in Chinese represents a wheeled vehicle. The Hebrew ג is supposed to have been derived from the outline of a camel's head and neck. The Chinese have a character Ko, which when joined to the radical Ma, "a horse," is pronounced Lo, and signifies "a camel." The same character Ko, when joined to the radical Chay (which is represented by G), signifies "wagon." The form G can be derived even more easily from the Chinese hieroglyphic representation of a vehicle [Pl. I, fig. 7], than the form ג from the hieroglyphic of a camel. There are many other indications that the third letter of the alphabet at first signified "a carrier."

l. Some of the Chinese literal analogues appear to furnish an onomatopoetic clue to the shape of the letters, that is wanting in the significance usually attached to the various ancient alphabets. When we are told that יוד means "hand," and כף "hollow hand," there seems to be no natural connection between the sound and sense. But when we find that in Chinese, Ya signifies "teeth; the parting branches of a tree; anything forked;" and that its hieroglyphic representative is Y, while Keen, "to gape," is represented by K, the natural position of the mouth when one is gaping, or calling attention to the teeth by signs is represented by the forked portions of those letters.

m. The Chinese characters are not all ideographic. Some of them are evidently combined phonetically, according to fixed rules of spelling, and others may probably, as M. de Guignes suggests, be composed of a number of alphabetic elements, that spell Hebrew, Phenician, and other ancient words. M. de Guignes claimed that he could spell, according to his system, over five hundred Chinese words, but unfortunately he appears to have left no record of any

except the few which he introduces to illustrate his memoir. By means of the few conjectural letters that he has given, a number of words may easily be found that tend to corroborate his views, and although the evidence that they give is by no means conclusive, it is sufficiently curious and interesting to tempt one to farther investigation.*

n. The Chinese writing contains all the elements of the alphabetic letters,—the horizontal line, the perpendicular, the oblique, the hook, the curve, the point,—and to each of these elements it attaches a special meaning. The same reason that leads us to infer the antiquity of an alphabet, from the fact that each of its letters retains a certain significance, would, *a fortiori*, indicate the still greater antiquity of a system that retains a meaning not only for every letter, but for every element of each letter.

o. Through the study of the Chinese hieroglyphs, the number of radicals may be greatly reduced, and an alphabet might perhaps be compiled, no more extensive than our own, from which all the characters of the language could be formed by combination, according to simple rules. The whole number of primitive hieroglyphs does not probably exceed eighty,† and many of these are found only in a few words. At least two-thirds of the words that are given in the Dictionaries of De Guignes and Morrison, appear to be made up of about twenty primitives.

p. It is reasonable to suppose that the earliest efforts at speech would be accompanied by expressive gestures, and that the earliest writing would employ images suggested by these natural gestures. We accordingly find, in all known systems of picture writing, that different portions of the human body occupy a prominent position. And all the organs which have names corresponding to those of the Hebrew letters,—the hand, hollow hand, eye, mouth, ear, head,

* Sir William Jones (Asiatic Researches, Vol. II, p. 373), says: "As to the fancy of M. DE GUIGNES, that the complicated symbols of China were at first no more than Phenician monograms, let us hope that he has abandoned so wild a conceit, which he started probably with no other view than to display his ingenuity and learning." This criticism, flippant as it seems in view of the distinguished scholarship of the French savant, is perhaps justifiable, but the curious coincidences that M. de Guignes has pointed out, especially those between the names of the early Chinese and Egyptian kings, are such as to render it still an open question, not whether *all* the Chinese symbols were Phenician monograms (which no one probably ever imagined), but whether any of them may have been originally formed after the manner of the Egyptian cartouches.

† All the most important ones are given in Pl. I, figs. 19 to 90.

tooth,—are represented in the Chinese hieroglyphics, under a gradation of forms, some of which agree with common forms of the corresponding alphabetic letters.

q. The hand was a prominent hieroglyph with the Egyptians as well as with the Chinese, and in each language it appears to have been employed in some form to represent the sounds of C, G, K, E, and T.

r. The most ancient alphabets exhibit either an entire absence or a dearth of vowel sounds, and it seems probable that the characters that subsequently became vocal were all at first consonantal. The alphabet that was carried by the Pelasgi into Italy, probably about 1400 B. C. [See Pl. I], contained only the vowels A, E, I; hence it has been inferred that these were the oldest vowels. As their forms may all be derived from Chinese characters signifying "foundation" or "support," the idea seems plausible that they were introduced after the invention of other letters, as supports or foundations for the sounds of the mute consonants.

s. Many of the Egyptian and Chinese hieroglyphics exhibit a close resemblance that appears to indicate a common origin. Instances of this resemblance may be found in the Chinese Rad. 8, Tow, denoting "top," or "head;" Rad. 10, Jin, which is usually interpreted "man;" Rad. 14, Meih, "a cover;" Rad. 17, Kan, "gaping;" Rad. 75, Muh, "tree;" Rad. 102, Teen, "field;" Rad. 119, Me, "rice." In nearly every case where both the Egyptian hieroglyphs and the Chinese characters exhibit an alphabetic resemblance, the Chinese resemblance appears to be the closer of the two.

t. Some of the hieroglyphs would represent the same letter in Egyptian and Chinese. Thus the Chinese Kan might be substituted for an Egyptian K [Pl. I, fig. 10]; one of the Egyptian representatives of M, resembles the Chinese Muh, "a tree" [Pl. I, fig. 12, No. 1], or Me, "rice" [Pl. I, fig. 12, Nos. 2, 3, 4]; the serpent L of Egypt [Pl. I, fig, 16], reminds one of the dragon Lung of China; the leafy Sh [Pl. I, fig. 14, No. 2], and the star S, find marked analogues in the Chinese Show [Pl. I, fig. 14, No. 1], and Sing. In one instance at least, two of the Egyptian forms for the letter M [Pl. I, fig. 12, Nos. 5, 6], seem to be accounted for by two forms of the Chinese Rad. 119, Me, "rice." [Do., Nos. 3, 4.]

u. If the supposed derivation of *H* and θ from the image of the sun (see Proceedings A. P. S., 1861, p. 8), is correct, an interesting harmony is traceable in the Egyptian, Greek, and Chinese, through the scarabæus which represented the sun, and was also used

for the letters τ and θ,—the rising sun, which was employed both for H and θ,—the Greek words Ἥλιος, Θεός,—and the Chinese figure of the sun, which resembled the θ in form. [Pl. I, fig. 32.]

x. The rounded shape of the mouth in pronouncing O and U, makes the open mouth, or the eye, or any other round object, a natural symbol for those sounds. In the Chinese forms that represent mouth, eye, and revolving, may be found fac-similes for nearly all the alphabetic representatives of O, U, and V. Even the curve or hook, which the Hebrew ו was supposed to denote, was represented in Chinese by one of the characters for mouth.

y. The Chinese may perhaps furnish a clue to some lost alphabetic forms, and some unexplained Egyptian symbols. Thus the ancient form of Z is said to have been 工. These two forms are both employed in writing the Chinese Kung, Rad. 48. [Pl. II, line 7, Nos. 3, 4.] The Egyptian symbol of life, the *crux ansata*, may be readily formed by placing the Chinese Sze, "self" (O), upon Ting, "to support" (T), as if to imply that which is living or self-supporting. This combination is actually employed in the Chinese Yu, "to give mutually." [Pl. I, fig. 15, Nos. 2, 3, 4, 5.] The Chinese characters, Shih, "tongue," [Do., No. 1], and Tsze, "child" [Do., Nos. 6, 7], have analogous forms, and the former is added to the radical Shwuy, "water," to form the word Hwŏ, "living; to vivify." The Egyptian winged sun [Pl. I, fig. 18, No. 4], may perhaps be etymologically connected with the Chinese Seih, "what existed in time past; anciently;" "custom; habit." [Do., Nos. 1, 2, 3.] The Chinese open mouth, "Kow" [Pl. I, fig. 17, Nos. 1, 2], has the same phonetic value, and nearly the same form as the Egyptian K. [Do., No. 3.]

z. In attempting to penetrate the mists of the pre-historical past, it is desirable to make use of every clue that may promise to furnish any guidance. Such a clue has been given us in the names of the Hebrew and Greek letters, and the significance,—partly well established, partly hypothetical,—that has been attached to those names. Plate II exhibits a few of the Chinese forms, which seem, both by shape and meaning, to have the most obvious connection with those employed by other nations.

1.* א. The Hebrew Aleph is said to have denoted "an ox; a leader; a prince." Some writers have supposed that the letter was

* The figures in the following paragraphs refer to the lines that are similarly numbered in Plate II.

modelled from the outline of an ox's head, a supposition that seems somewhat plausible, if we examine the head of the hieroglyphic ox-hide in Pl. I, fig. 72. But the Chinese archetypes in Pl. II, line 1, all of which appear to denote either foundation, beginning or head, seem to furnish a more marked correspondence with the most ancient forms of the first letter of the alphabet.

2. ב. Beth, "a house; a place; a box." The Chinese archetypes in the second line denote, 1, "a receptacle;" 2, "an inclosure;" 3, 4, 5, 7, "a mound;" 6, "a house."

3. ג. Gimel, "a camel." The 8th, 9th, and 10th forms in line 3 of the archetypes, are derived from Rad. 16, "a support," and Rad. 8, "a top, or head," denoting "that which supports the head; the human neck; the throat; strong; unbending;" &c. The remaining forms denote either Ting, "to carry," or Keu, "a carriage."

4. ד. Daleth, "a door." The 5th and 6th of the Archetypal forms represent a door; the 1st and 7th appear to have been derived from the outline of a knife or hatchet; the 2d, 3d, and 4th denote a mouth or opening. The 18th Chinese radical is Taou, "sword; knife." [Pl. II, l. 22, fig. 6]. The form of the radical is strikingly like that of the Hebrew Tau, and its name furnishes the nearest monosyllabic approach that the Chinese can make to the pronunciation of the Sanscrit root **dal**, "to cut; to divide."* The Hebrew טלע, "to cleave," appears to connect these several meanings, and to render it probable that the earliest hieroglyphic representative of the dental sounds was either "a cleaver," or "a cleft."

5. ה. He, "hollow." The archetypes appear to be all traceable to different modifications of Rad. 16, Ke, "niche; support;" and Rad. 21, Pe, "ladle."

6. ו. Vau, "hook." The Chinese forms signify either "hand," "claw," "angular," or "hooked."

7. ז. Zayin, "armor." The 3d and 4th archetypes are forms of Rad. 48, Kung, "work; art." This radical is sometimes represented by a hand. The others may be derived either from the hieroglyph of a hand, or of something carried in the hands,—as a bow, a child, or a shield. That the primitive idea was that of carrying, is rendered the more probable from the relative positions of the Latin, Hebrew, and Greek letters, C, ג, *Γ*, and G, ז, *Z*.

8. ח. Hheth; meaning doubtful. The Chinese forms may be derived from "table," and "sun."

* Compare Ger. *thal*; Eng. *tale, deal.*

9. ט. Teth, meaning unknown. The archetypes are all employed to represent the sun.* Forms somewhat similar are sometimes used for the mouth. The Egyptian character for "splendor," is sometimes written nearly like ט, and sometimes nearly like the fourth archetype.

10. י. Yodh, "hand." The third archetype sometimes signifies "to put aside; to reject." The other three are different forms of the hieroglyphic hand.

11. כ. Kaph, "hollow hand." The Chinese characters signify "hollow; opening or separating; branching." The first and second forms are sometimes used for a hand in the act of grasping; the fifth, which is one of the modifications of Rad. 75, Muh, "a tree," appears to be the archetype of the sixth and seventh alphabetic K's.

12. ל. Lamedh, "to instruct; expert." The archetypes are all forms of Rads. 9 and 10, which are both called Jin, and are both evidently modifications of a single radical. The ordinary definitions are "man; high;" but "top; head or covering," appears probably to have been the primitive meaning. The same primitive often assumes the form of R [Pl. II, l. 20, figs. 1, 3]. The phonetic connection of L and R, renders it probable that the ideas of "instruction" and "head" may have been associated in the minds of the inventors of the alphabet.

13. מ. Mem, "water." The first three Chinese forms denote "water" or "a channel." The fourth and fifth represent a bud or shoot. The sixth is one of the forms of Rad. 119, Me, "rice," to which reference has already been made.

14. נ. Nun, "fish; snake." Archetypes 1, 2, 3, 4, and 6, are different forms of the Chinese character Nae. M. Abel-Rémusat,† in a letter to Baron Humboldt, treats somewhat fully of its various meanings, all of which seem to involve the idea of flowing, connection, or continuance. The third and fourth forms might easily be imagined to represent fishes or snakes, but it seems more probable that they were derived from the hieroglyph for "water," which is one of most common Egyptian representatives for the sound of *n*. In some Chinese words (the word King, for instance), water is depicted under a gradation of forms, some of which are precisely like the Egyptian. The Egyptian hieroglyph for "Nile" or "river," is made up of two characters, one of which resembles the Chinese Nae, and

* V. *ante, u.*

† Nouveau Journal Asiatique, Vol. XI, pp. 273–282.

the other may have been intended for a receptacle or mouth (Coptic Lo). It may, therefore, have been designed to express phonetically the Greek word *Νειλος*. The curved stroke at the right of the sixth form, which corresponds precisely with some of the most common alphabetic N's, forms also the principal part of the Chinese characters Kaou, "air, vapor," and Yen, "a long journey." The fifth figure, Sin, "a heart," would, according to the rules of Chinese orthography, represent a final N, and it may be the archetype of the heart-shaped N in the Punic inscriptions.

15. ס. Samech, "prop." The first figure in line 15 represents a prop or support; the others are forms of the word San or Sam, "three."

16. ע. ʿAyin, "eye." The Chinese word Yen, "an eye," may be the root of ʿAyin. The third archetype is one of the most common hieroglyphs for the eye; the others denote either the mouth, or whatever is round or rolling.

17. פ. Pe, "mouth." All of the Chinese forms in line 17 are employed to represent the mouth. In the Chinese word Too, the primitive which denotes "mouth" or "inclosure," is written in the various forms of ב, פ, P, B.

18. צ. Tsadhi, "locust?" Tsaou Chung, in Chinese, signifies "a locust." The archetypal forms represent a sharp, shrill, "cutting" sound.

19. ק. Koph, "ear." The Chinese characters represent either "ear" or "orifice."

20. ר. Resh, "head." The archetypes are all found either among the forms of Rad. 181, Heĕ, "head," or in the outlined heads of animal hieroglyphs. [Pl. I, figs. 63, 64, 78.] The word Heĕ is as near an approach as the Chinese can make by a single utterance, to the pronunciation of the Hebrew monosyllable Resh. The character P, which is found in lines 17, 18, 19, and 20, is usually employed to represent Rad. 26, Tseĕ. The Tseĕ was an instrument of stone, horn, or bamboo, by which officers were appointed or authorized to act. After having letters engraved upon it, it was cut through the middle; one half was retained at court, and the other given to the person appointed. The same character is sometimes employed for Fow, "a mound," and for Chung, "the middle; within; half," and it forms the principal part of the word Ling, "to order; to enjoin." Chung is commonly represented by a mouth divided by a line passing through its centre.

21. ש. Shin, "tooth." The archetypal resemblances do not point

so strongly to a single primitive, as in the case of some of the other letters. Analogues are found in the characters which represent mountain, mouth, teeth, arm, and water. The alphabetic resemblances between the various forms of M and Sh (lines 13 and 21), would seem to point to "water" as the earliest symbol of the two sounds.

22. ת. Tau, meaning doubtful. The idea of cutting or piercing, appears to be conveyed by each of the Chinese forms. (See remarks on line 4 above.)

EXPLANATION OF THE HIEROGLYPHS ON PLATE I.

(For Nos. 1 to 18, see references above.)

19. Taou. A knife.
20. Tseïh. A battle-axe.
21. Kow. A mouth.
22. Yew. Hand.
23, 24. Yue. Moon.
25. Neu. Woman.
26. Tsze. Child.
27. Shan. Mountain.
28. Chuen. Channel.
29. Kung. Bow.
30. Sin. Heart.
31. Wăn. A painting.
32. Jih. Sun.
33. Muh. Wood.
34. Moo. Mother.
35. Ke. Vapor; spirit.
36. Shwuy. Water.
37. Ho. Fire.
38. Pan. A support.
39. Ya. Tooth, tusk.
40. Neu. Ox.
41. Wa. A tile.
42. Teen. Field.
43. Pih. White; pure.
44. Ming. Dishes.
45. Muh. The eye.
46. Ho. Grain.
47. Leih. To erect.
48. Me. Rice.
49. Chuh. Bamboo.
50, 51, 52. Wang. Net.
53. Yu. Wings.
54. Urh. Ear.
55. Juh. Flesh.
56. Chin. Official cap.
57. Pe. Nose.
58. Kew. Mortar (v. 86).
59. Shih. Tongue.
60. Tsaou. Herbs.
61. Chuen.* Boat.
62. Yen. To speak.
63, 64. Shin. A body.
65. Keu. A carriage.
66. Yew. Liquor.
67. Mun. Doors.
68, 69. Chuy. A short-tailed bird.
70. Yu. Rain.
71. Fe. False (v. 74).
72. Kih. A hide.
73. Heĕ. Head.
74. Fe. To fly.
75, 76. Show. Head.
77. Heang. Fragrance.
78, 79. Yu. Fish.
80. Neaou. Bird.
81. Mang. Frog.
82. Ting. A tripod.
83, 84. Tseŏ. Sacrificial cup.
85, 86. Che. Teeth.
87, 88. Lung. Dragon.
89, 90. Kwei. Tortoise.

* The first form is Chinese, the second Egyptian.

In the varied character of the resemblances that have been thus briefly pointed out, extending, as they do, not only to all the customary forms that are found in memorial inscriptions, but also to the modern running hand, there seems to be a mass of circumstantial evidence, which leads almost irresistibly to the conclusion that the whole history of the invention and gradual perfection of alphabetic writing, must be still preserved in the literature and monuments of China. Of the antiquity of the Chinese Tsaou Shoo, or cursive script, and the recent introduction of similar forms into our own writing, there can be little doubt. It seems to be established beyond any reasonable cavil, that the former has been in use for at least two thousand years. There is a noteworthy coincidence between the date of the Saxon running hand (in the eighth or ninth century), and the Augustan age of Saracen literature and empire, which renders it probable that the learned Mahometans may have communicated to the scholars of Europe, a knowledge of the rapidly-formed letters that had long been used in Asia, and that the advantages arising from their use were so evident as to lead to their speedy general adoption.

Extracts from a letter were read from Prof. J. D. Whitney, geologist of California, relating to the survey of that State, promising the publication soon of one or two valuable volumes of reconnoissance, to be followed by special reports in due time, at the order of the Legislature. "Our results," Mr. Whitney writes, "are, I think, likely to interest the geological world quite strongly. We have found the geology of California to be very different from what it had been represented to be by the Pacific Railroad geologists." Mr. Whitney expects to spend the spring months in additional field-work in the Sierra Nevada, before publishing.

Professor Lesley communicated a notice of a remarkable coal mine or Asphalt vein, cutting the horizontal Coal-measures of Wood County, Western Virginia.

Mr. Lesley said, that through the kindness of R. H. Gratz, Esq., of Philadelphia, a descriptive letter and a map had been submitted to him, which exhibited geological facts of more than ordinary interest to those who are studying the origin of the rock-oil deposits of the West. This letter agrees with previously received, but vague, reports of a true vein of bituminous coal or bitumen. The curious points of the case require careful investigation; but there seems to be no good reason to doubt the essential correctness of the statement.

The mine is situated on a four hundred acre tract of woodland (oak, elm, maple, walnut, &c.), the position of which, in relation to the rivers and railroad of the neighborhood, will be best shown by the accompanying map. Plate III. It may be well to premise a few words about the coal-measure region in the heart of which it lies.

By referring to any map of all Virginia, it will appear that the North and South Branches of Hughes River unite and flow into the Little Kanawha about thirteen miles (in a direct line) above its junction with the Ohio at Parkersburg. The mine itself is somewhat over twenty miles (in an air line) southeast of Parkersburg, and a little under eight miles in an air line, south 4° west (both true and magnetic), from the bridge of the Parkersburg Branch of the Baltimore and Ohio Railroad over the North Branch of Hughes River.

Two peculiarities mark this "coal vein." 1. It is vertical, while all the stratification of the country is nearly horizontal; and strikes S. 78° W. (N. 78° E.), whereas the strike of the country is S. 35° to 40° W. (N. 35° to 40° E.) 2. It is a solid bitumen-vein rather than a coal-bed.

1. The country of the neighborhood is that of the central part of the great synclinal, which crosses the Ohio below Pittsburg, and stretches down through Western Virginia parallel to the Ohio River, into Eastern Kentucky. Across this broad and flat synclinal of coal-measure rocks there flow from southeast to northwest, to fall into the Ohio successively, beginning at the north, the branches of the Little Kanawha, of the Great Kanawha, the Guyandot, the forked branches of the Great Sandy, (and then in Kentucky) the headwaters of the Kentucky, the headwaters of the Cumberland, and finally in Tennessee, the headwaters of the Caney. All these fan-shaped water-basins have their highest or southeastern limit defined

by the strike (N. E. and S. W.) of the more upturned rocks of the southeastern side of the synclinal. With the exception of the Great Kanawha main stream, a line drawn along so as just to touch the extreme tips of all the outermost twigs of these water-trees, will give the southeastern limit of the great Alleghany Mountain or Cumberland Mountain coal area. Their waters collect in flowing northwest, break through the central measures and higher coals of the synclinal, and either join the Ohio (which flows along the depression between the upper and lower coal systems of the True Carboniferous), or the Kentucky and Cumberland Rivers further south.

From this short description it may be inferred, and it is a correct inference, that this belt of synclinal, is in great measure an irreclaimable mountain wilderness; a labyrinth of narrow hog-back ridges and steep, deep, winding vales, providing spaces for agriculture only along the narrow margins of the principal streams, and at here and there a little upland plain, caught in between the headwaters of half a dozen fan-shaped systems of drainage; but all the rest covered with an everlasting forest, folding over the furrowed face of the earth. The region consists in fact of myriads of secluded glens, surrounded by stair-like cliffs from four to eight hundred feet in height, and separated by spiculæ of mountain, which shoot out from the more central water-divides, like crystals of ice over the surface of a pool. The extremely tortuous course of the principal streams is illustrated by the map. They do not flow from side to side of wide, flat valleys, but around sharp mountain prongs, which point across towards opposite open ravines or valleys of considerable length. These prongs descend from the dividing high lands, like the spurs of the Pyrenees from the central ridge, but in long steps, the strata being nearly horizontal, and each sandrock in the descending order carrying the nose out further than the one above it. Narrow terraces carry the outcrops of the long steps of the nose, round each side of the prong along the steep side of the valley.

The coal-beds pass horizontally through the pronglike ridges from valley to valley. Some of these ridges run as narrow on top and as regular as railroad embankments, for three or four miles, and in nearly straight lines, between equally straight vales terminating bowl-shaped against some cross ridge.

It is across such vales and dividing ridges, that the Asphaltum vein of Wood County makes a straight course, A B upon the map, "2323 feet long, as at first measured, but since then traced in both directions still further, so that now it is known to extend more than

two-thirds of a mile." Explorations beyond this line have failed to find it. Its outcrop, four feet ten inches thick, was discovered crossing a ravine fifty feet wide at the bottom, and rising on each side with slopes of nearly forty-five degrees. On one of these hillsides at a height of ninety feet, the outcrop showed the same thickness, but at a height of one hundred and eighty-five feet, it was found to be but two feet six inches thick. It is not certain that this diminution is in a vertical direction; it may be lateral; for the slope between the ninety and the hundred and eighty-five feet levels is more gradual, especially upon the western side.

In the bottom of the ravine, a vertical shaft was sunk to a depth of thirty-four feet upon the vein, which continued uniformly four feet ten inches thick, the asphaltum being filled in, pure and clean, without the least admixture of earthy or foreign ingredients, between the smooth and almost perfectly vertical walls of yellowish-greenish sandstone, lying in horizontal layers, through which this gash or fault was once no doubt an open fissure, communicating with some reservoir of coal oil, which still, it may be, lies beneath it undisturbed. The most interesting part of the phenomenon for structural geologists is this gash.

2. The substance which fills this gash-fault in the coal-measures of Northwestern Virginia, resembles the glossiest, fattest caking coals, and has a decidedly prismatic structure; breaks up into pencils, with flat, lustrous faces and sharp edges, but the faces not set at any fixed angles to each other; so that the effect upon the eye is rather that of a fibrous than of a prismatic structure. At the same time there is not the slightest appearance of layers, but the aspect of complete uniformity or homogeneity. Pieces are taken out, it seems, a foot in diameter; and that portion of one of these pieces which I have, shows a plain face on one side, as if it had encountered one of the walls, and is covered with a delicate film of a dead black substance like charcoal dust, which is probably the dust of the vein substance itself.

"Pieces lying at the surface of the ground are said to yield as much oil as specimens taken out six or eight feet down. By the ordinary dry distillation, the substance is reported to yield as much oil as the Albert coal. By a different process, the first and only trial, at which 600 pounds in one charge was used, 44½ gallons of superior oil was obtained. Retorts are now upon the ground."

By an assay made by Mr. B. S. Lyman, of Philadelphia (the amount of hydrocarbon soluble in benzole being about one-half of the

whole) the volatile matter (mean of two assays) was 47.11 per cent., Coke (52.71, 53.07) 52.89; Ash (1.65, 1.81) 1.73.

There seems to be no escape from the conclusion that the substance filling this vertical vein is a product of the gradual oxidation of coal oil once filling the open fissure. It is not impossible therefore that the lower regions of the fissure are still filled with liquid oil; and that we may see in this instance an illustration of the condition of things far beneath the surface of the coal oil regions of Western Pennsylvania and Eastern Ohio. The vast quantities of oil delivered by the flowing, the blowing, and the spouting wells require fissures of this kind, either never opened up clear to the surface, or else once opened and now reclosed, or else filled in with detritus. The different depths at which closely neighboring wells begin to spout or to flow, oblige us to imagine similar fissures at oblique angles. If Sterry Hunt's hypothesis be accepted, that the Corniferous Limestone is the mother rock of the oil, such fissures become still more needful to bring the oil to the surface, from the vast depths at which the Corniferous Limestone underlies the True Coal-measures.

Vanuxem first described the films and buttons of "anthracite," as he called it, with and in the quartz crystals of the Calciferous Sandrock of New York, at the base of the Silurian system. Mr. Hunt describes the veins and fissures of all the limestone, shale, and sandstone members of the great Quebec Group (which is the enlarged equivalent of the Calciferous in New England and Canada) as frequently either lined or filled with a similar substance. Sometimes the varnish lining has cracked in shrinking; sometimes botryoidal masses of it have been left; sometimes hundreds of pounds of it are packed away solid in the crevices. In one exceedingly instructive case the vein of bitumen, inclosed in walls of rock crystal, is itself cut by thin seams of quartz.*

* Hunt in Amer. Journal, March, 1863, p. 163. The force of the argument deducible from this fact, against the igneous, and in favor of the aqueous production of our quartz veins, will be felt at a glance. I cannot but express my surprise that Sir David Brewster should continue to claim as an argument for the igneous theory, the presence of two different elastic hydrocarbon fluids in cavities in topaz, beryl, and diamond, especially in regard to the permanent compression they have effected in the molecular structure of the walls of the cavities, as detected by polarized light. (Trans. R. S. Edinb., XXIII, i.) Yet M. Fournet supports his argument. (Comptes-rendus, LI, p. 42, LIII, pp. 83, 610; Geol. Lyonnaise, Lyons, 1861, pp. 533, 715, quoted by Sir David Brewster.) While M. Elie de Beaumont rests for its refutation on the volatility of the fluids, and the frequency of fluid-cavities in all quartz gangue rock. (Comptes-rendus, LIII, p. 83.) Sir David Brewster says that M. Fournet "has removed this difficulty" (Geol. Lyon., p. 536), but does not say how.

In these older instances of bitumen veins, we see small prototypes of the large vein under consideration.

The point of the phenomenon most interesting to structural geologists is this: Two opposite deductions are possible from the facts as made known, on the one side in favor of the vast antiquity of the coal oil, and on the other side in favor of the recent denudation of the surface. If we have in this vein a deposit of coal oil hardened by time and the absorption of oxygen, it is certain that the cutting out of the ravines across which it lies, must have taken place subsequently; for the outcrop rises to a height of nearly two hundred feet on each side of the bottom of the ravine in which the shaft is sunk. I do not learn from the report whether detached blocks or pieces of the bitumen occur upon the surface, or in the alluvium of the vale below the crossing of the vein. But that is of no consequence to the principle. The valleys which it crosses must be younger than the vein, if the vein was filled with fluid oil. Hunt shows plainly (see Sill. Journ., March, 1863, p. 167), that the oil which fills the fossil casts of particular exceptional strata in the Lower Devonian Formation (as in Bertee Township on the Niagara River opposite Buffalo), must be an original deposit, and not a subsequent infiltration or exudation, inasmuch as it has lined with oxidized bitumen the cavities of the fossil casts in this stratum, and not those in similar strata above and below.

All that we know of the grooving of the surface of our palæozoic areas consents to the great antiquity of the action, whatever that action may have been. To demonstrate the antiquity of the Corniferous coal oil, is merely to give more room for the antiquity of the oil. Yet, the denudation, however ancient we may make it, must still be kept more modern than the antecedent formation of the coal oil and its change to bitumen.

The date of the formation of the oil may be placed anywhere beyond the close of the Palæozoic era, even as far back as the beginning of the Devonian, or even in Lower Silurian times; since the Quebec Group is also the home of oil. The denudation of the surface of the coal areas cannot of course be put back beyond the uplift of that area into the air.

There remain two hypotheses for dating this denudation. One class of geologists, the Cataclysmists, give the date of the uplift as the date of the denudation; make the two phenomena related and dependent parts of one great action. The other class, the Secularists, regard the present face of the country as but the latest phase of an

infinite series, beginning at the uplift and still in progress. An intermediate view, held perhaps by some eclectics, supposes a succession of denuding actions of unknown force and indefinite number.

As to the Appalachian region of the United States, I think that the principal special objection to the theory of one cataclysm (apart from general considerations) has not been mentioned, or at least clearly stated. And yet it seems to me of great force. It is a deduction from the fact that the estuary bed of the New Red deposit, taken as a grand whole, can hardly be regarded otherwise than as a part of the Post Carboniferous denuded surface, and therefore subsequently formed to the great cataclysm supposed by that theory to have produced that surface. For the surface of the New Red is eroded exactly in the style, and in the direction of, and in entire harmony with the erosion of the surface of the Coal; which of course would make the supposed cataclysm subsequent to both. Two cataclysms being therefore required, a new difficulty appears.

Supposing the first cataclysm to have eroded the palæozoic areas, so that the deepest valleys of erosion nearest the Atlantic seaboard could be filled in with New Red deposits, why were these deposits restricted to the New Red estuaries, so well known as to need no description here? Every one is aware that New Red is nowhere seen behind the range of the South Mountain or Blue Ridge. Yet there are plenty of gaps wide and deep enough to let it through. If it had ever been deposited in the great Lower Silurian Valley behind that range, no cataclysm can be supposed to have acted with such consummate skill and completeness, that not a hillock or corner bit should have remained to tell the story of where its outspread masses had originally lain.

If now, to meet this difficulty, the Cataclysmist brings down the date of his first agency to Post Secondary days, and imagines the New Red rocks to have been excluded from the Great Valley because in fact, no such valley, and no gaps leading into it, had as yet been formed, he not only encounters the old difficulty of providing its estuary bed for the New Red, but in addition to that, the awkward statement that the gigantic anticlinals of the Palæozoic age, once made, remained, uplifting their more than Andean or Himalayan masses in the sky, (with all the climatal consequences of such a supposition), during all the ages through which the so-called Permian of Kansas, and the New Red, and the so-called Oolite of the Atlantic seaboard, were depositing their layers.

And when he has settled all this properly, the discussion will re-

open upon him in the same form anew, so soon as the denudation of the Cretaceous and Tertiary surfaces come to be regarded as in like manner in harmony with those of older dates.

At Cornwall, six miles south of Lebanon, hills of New Red Sandstone, three, four, and perhaps five hundred feet high, stand, looking in upon the great Silurian plain, like Peris at the gates of a Paradise they cannot enter. If along this line a *fault* has in fact carried the New Red down to the present level of the Silurian plain, the denudation of the two surfaces is nevertheless so far one phenomenon, that in its present condition it is to be explained by reference to actions subsequent to the deposit of the Conglomerate, or uppermost New Red layer, the so-called Potomac marble. But the hypothesis of a fault along the south base of the South Mountain is a pure fiction of embarrassment. If it existed anywhere, it must extend several hundred miles, and be approximately a straight line. The most cursory glance at the geological map of Pennsylvania will satisfy any one that no such fault exists. The succession of spurs of the mountain range forbids it. The gophered edge of the New Red on the Lancaster County limestone forbids it, and shows how entirely superficial the New Red is. No river section shows the fault. It is a pure fiction. The northwest dip of the New Red against the Azoic mountain range is still a problem to be solved.

The hypothesis of suboceanic erosion, contended against by the geologists of the United States almost from the beginning, is fast losing, if it has not lost altogether its hold upon the European mind. The conviction is well established, which we freely expressed years ago, that the ocean is a builder and not a leveller. Like the quietistic and subjective letter M, which was its symbol in ancient literature, the main, the murmuring Typhon, has always been the absorber, and the mother of multitudes. While the fringe of foaming breakers, the Herculean Hydra, and in fact all river water, the rushing and hissing Typhon, of which the letter S was symbolic, has always been the destroyer, the enemy of the established, the ravager of the surface. It was upon this basis that some *subaerial* cataclysmic hypothesis like that of Professor Rogers came to be favored by those who knew the grandeur of the work which had been done by the denuding force whatever it was, among the palæozoic anticlinals of America; and who felt the perfect harmony which reigned over the whole expanse of the phenomenon, from the Tertiary seaboard of the Atlantic and the Gulf, past the beds of the great freshwater Devo-

nian and Silurian lakes, to the original shores of the Laurentian Continent.

We cannot regard, therefore, without some natural chagrin, the latest treatment of the subject by Professor Tyndale and Professor Ramsay, of England; for these accomplished observers not only take up our own old views with all the empressement of new discoveries, but make what seems to us the very absurd attempt to carry the petty energies of mountain floods and local glaciers up to the work of excavating, not merely lakes like those of Como, Constance, and Geneva, but such seas as Lake Huron and Lake Superior. It is gratifying, however, to see that such views can be refuted by European observers, who have never encountered the phenomenal problems of America. The impossibility that a moving glacier after descending to sea level, should excavate the bed of a lake, and continue to move up and over its farther end, even taking the smallest Alpine lake known for an example, is admirably demonstrated by Mr. Ball in the February number of the London, Edinburgh, and Dublin Philosophical Magazine for 1863. If this be not possible for the tarns among the valleys where glaciers are at home, how can it be possible for lakes and seas, where the existence of glaciers at any epoch is a theory? And how reckless of all consequences must that theory be, which reads an incantation to these icy demons, to accomplish the symmetrical erosion of a triangular area of earth-surface a thousand miles on each side long, the southern angle of which touches the parallel of 33°!

Professor Ramsay calls attention to the remarkable fact that the lakes of Europe and America seem to be confined to the scratched and grooved portion of the hemisphere, and that they are not found further south than the drift, except in Alpine, that is to say, in glacial regions. This is a coincidence, indeed, which ought to harmonize the two phenomena under some theory; but not necessarily subordinate the one to the other as effect and cause. I have no satisfactory explanation to give for the coincidence. The special reasons for the existence of each separate lake can be easily pointed out. The damming back of the waters of the New York Devonian lakes, including Erie and Huron, are due to the gentle northward rise of their floor-rock. Lakes in the same soft Devonian measures, are numerous along the valley of Pennsylvania, at the foot of the Alleghany Mountain, but only where the measures are gently inclined. Lakes disappear from the map as the eye passes southeastward over the more upturned regions. Steepness of dip is hostile to deep excavation.

The reverse is true of erosion above water-level. Steepness of dip is favorable to aerial disintegration, to the dissection of stratification, to the subdivision of one massif into several, and of one hillock into many; hence to the general degradation of the surface under air. But under water the reverse is true.

In the Laurentian and Huronian, Scandinavian or Azoic regions of the North, where distortion and plication have revelled from the beginning to reduce things to anarchy, and where alternate potash rocks and limestones form the boldest contrast of endurance and decay, lakes abound. A clean, smooth drainage system, worked out so completely (without stating the agency) as to leave no holes, nor cul de sacs pointing in the wrong direction, nor crooked lakes, is possible only when the stratification is clean and in good order, cutting equally and smoothly in all directions according to the force, and permitting the law of compensation to have free course in the establishment of a common and gently declining niveau of reference to water-level. But any conceivable erosive agency, cataclysmic or secular, must encounter a million contretemps, in smoothing off its work over a country like Canada, where no outcrop runs far without doubling like a hare. Sir William Logan has shown that the crooked lakes and lake-like rivers of that country conform to the plications of the primary limestone belts.

Mr. Ball's own hypothesis of an original fault structure for the lake system of the Alps is not new, and is open to as much objection on other grounds, as the theory of Professor Ramsay which he overthrows. If applied to the Devonian lake system of New York and Pennsylvania, and therefore, of course, to the thorough-cut valley system of the Carboniferous plateau of the Alleghany Mountains of Northern Pennsylvania, it will not find a fact to stand upon. Not a trace of fault structure is to be seen over all that immense region; yet the erosion is in straight lines, north and south, and from five hundred to a thousand feet deep. Also not a trace of *original* glacial action can be found. Diluvial striæ are rare; moraines and taluses are wanting. Not one has yet been recorded, if any exist, nor have I ever seen throughout that region, any resemblance to one which did not resolve itself on examination into a barrier outcrop, slightly masked by soil or local drift; and even instances of this kind are rare.

On the other hand, throughout that whole region, the Lyellist can find no evidence of a slow wear and tear through the ages. The region is swept too clean for that. There are no piles of detritus, no

cones at the mouths of ravines, no plains of sand and clay, no deltas at the embouchures of streams and heads of lakes, such as, in the Auvergne, and in the Alps and Pyrenees, impress the traveller with an instantaneous and irresistible conviction of slow wear and tear. On the contrary, the walls of the valleys, high as they are, are vertical bluffs, alternating with taluses of angular blocks fallen from them; the bottoms of the valleys are clean; the lakes have steep shores, and the plains are covered with the disintegration of their own rocks. Everything one sees tells one story, and that the story of a cataclysm which, at one sweep, accomplished valleys, plains, and lakes, leaving next to nothing for all coming time to do, but to protect the surface with vegetation, and to send an annual contribution of the meanest value by the rivers to the sea.

Two systems of valleys characterize the result, as we now study it. One parallel with the coast, and produced by the sweeping away of the tops of anticlinals from one to twenty miles wide and miles in height; the other a transverse system of river bottoms, sunk some few feet or yards below the longitudinal valley which they cross, and of deep, clean, straight gaps through the bounding mountains. It is demonstrable that these two systems are but two parts of one, and owe their origin to the same agency, and at essentially the same time. The peculiar relationship of the rivers to the gaps is sufficient of itself to prove this. Not a fault has been demonstrated in any of these gaps. One fault transverse to the Tussey Mountain occurs *near* one gap, that of the Juniata, and as if, by its loneliness and excentric position, for the express purpose of excepting to such a theory, if at any time one should be presented. It is not until the geologist has passed through the whole region, and has reached its southeastern limit, that he suspects a faulty gap. The Kittatinny or North Mountain is said to be faulted at the Delaware Water Gap, and at the Susquehanna; but so the Sharp Mountain was said to be faulted at the Swatara Gap, until careful instrumental work proved that the coal-beds on each side of the gap were not a hair's breadth out of line. A fault at the Susquehanna is evidently absent, for the very outcrops of the different sandrocks can be traced, at low water, from side to side. And the fault at the Delaware Water Gap is, I believe, nothing but an effect of perspective upon the eye, produced by the inclined lines of cliff, unsymmetrically wrought out on the two sides of the gap, because the cutting force worked in a curve, produced by the presence of the expiring Red Hill anticlinal on its northern slope.

No. The excavation of the Appalachian surface has not been determined by transverse faults; but entirely by longitudinal flexures; and has not been accomplished by glaciers; nor by rain and river water; still less *sub oceano*. By what then? I think much must be discovered before the question can be answered, if we reject subaerial deluge action. What for example do we know yet of the internal structure of those deep diluvions or alluvions which occur in our transverse river-bottoms, where they cross the longitudinal valleys of Devonian olive shale? They seem to be ancient lakes, excavated at the time the topography of the valleys and mountains was determined, and filled with river trash. As they occur in the transverse river valleys, they seem to own the rivers for progenitors. But being in line with the gaps, the occupation of them by the rivers seems, on the contrary, to be as fortuitous as the river-occupation of the gaps. Moreover, the present rivers are evidently the degenerate representatives of grander floods, and the silt of these depressions, judging by the surface, is of too gross and hasty a nature for collection by less than such original deluges. But supposing this also to be a fancy, what relation does the glacial hypothesis, which presumes to annul the necessity for a cataclysmic eroding agent, propose to bear to parallels of latitude?

Wherein does the valley of the New River or Kanawha differ from that of the Susquehanna or Delaware, except in having no New York corals or Canada syenites among its pebbles. In every structural feature they are alike; and like the valley of the Tennessee in Alabama. There is no change in the height or constitution or form of the mountain plateau through which they cut. There is no change in the range to the southeast of them which can affect the question; for the Black Mountains of North Carolina, even if liable to suspicion as glacier-bearers, are far enough removed from the New River on the north, and the Tennessee on the south, to be of no account in this discussion. Is the glacial hypothesis prepared to defend its claims in Middle Alabama under the parallel of 33°? If not, then it has no claims to any feature of the Catskill Mountains under the parallel of 43°, except their scratches; to which, so far as the genesis of mountains and valleys is concerned, it is quite welcome. Yet precisely this bonbon Professor Ramsay refuses it; for he maintains (against Dana), that the striæ at the Catskill Mountain House were made by icebergs floating down the Hudson estuary, and not at all by glaciers. There is a disposition manifested of late among the American geologists, of the New England school, to fill each of the

great valleys of the North with a great glacier of its own, naming them the Penobscot Glacier, the Connecticut Glacier, the Hudson Glacier, the Mohawk Glacier, the Susquehanna Glacier, &c. In view of Kane and Hayes's discoveries of the present state of things in Greenland, and for easy accounting not only for such groups of east and west and north and south striæ as appear at Cherry Valley, the Catskill House, and Wilkesbarre, but also for those which cross the polished summits of our highest mountain tops, such as the Penobscot Knob which looks down upon the valley of Wyoming, there is not the same objection felt now as was at first expressed against the Agassizan cope of ice for the hemisphere. President Hitchcock finds its reliquial glaciers in the valleys of Hampshire and Berkshire, and Professor Dana explains the absence of moraines now by the absence of any aiguilles to overhang and shed their stone-slides upon the back, or upon the edges of its subdivided streams.

The thus admitted absence of moraines, and the excuse advanced for it, return us unexpectedly to the starting-point of the discussion, the question. Could ice have fashioned our topography? No one doubts its ability to scratch and groove and polish. Can it excavate? And if it can, what is the limit of its excavating power? Leaving the glacialists of the fixed-ice school and the floating-ice school to settle between them the force, frequency, direction, and exact *modus operandi* of striation, quite sure that they will at least agree on the *date* of the phenomenon *as very recent*, we are left at liberty to revert to those more remote days, when the broad-backed anticlinals rose into the sky higher than any Alpine aiguilles or Andean volcanic cones; to speculate on, 1, Whether they were unbroken vaults; or split along their axes; 2. If split, whether split completely down to water-level, or how far; 3. Whether glaciers could have been then formed at all; 4. Whether, if formed, they could excavate a valley five or ten miles deep into the heart of an unbroken anticlinal; or 5, Do more than polish the central gorge, if the anticlinal were broken; 6. How such a central glacier could escape from such a gorge sideways, or in any direction but endwise, at the limits of the crack; or 7. Fail to leave high walls, alpine ranges, peaks, aiguilles, and moraines behind it when it disappeared.

Surely the glacialist must startle back from such an incredible picture. The great obstacle in the way of topographical science among geologists, has been an innocent ignorance of the titanic postulates upon the ground; and therefore, an inability to reconstruct in imagination the awful vaults of rock which have been removed from over

at least fifty thousand square miles of the surface of the United States, merely along the one belt of the Appalachian Mountains, between the great coal area and the Blue Ridge range. What has removed these massifs? The excavation of a hundred Lake Superiors to the depth of two thousand feet would not present the same difficulties. Either a cataclysmic subaerial deluge mighty enough to do the work, or a series of such deluges following each other until the work was done, or the atmospheric agencies at work on every square inch of the whole area for almost an infinity of ages,—one or other of these three must be the accepted force. Ice may come in for its share of the byplay, at various and very early times (as Ramsay has made probable in Shropshire) as well as in the last days of its glory, the stamp of which we see left upon our outcrop surfaces; but to make it the initial agency of such erosion is absurd. The power of ice could no more have swept those symmetrical palæozoic arches into the Atlantic, than a child could have flown to Loretto with its church. But whatever did accomplish that work, did it all; established the general register of heights; made every mountain a consistent part of the harmonious whole; worked out all the Lower Silurian valleys precisely on one pattern; excavated every Devonian lake from Harvey's Pond to Lake Huron alike; and cut to the same contour the subcarboniferous cliffs along the whole line from the icy Delaware to the sunny Alabama.

Of the seven or more chief points of speculation cited above, that of the split anticlinal is of course the most important. The admirable illustrations of the Austrian survey, which Haidinger and his noble coadjutors have been giving us for several years, repeat with variations all the curiosities of our Appalachian anticlinal structure, which were prepared for publication twenty years ago by the State Survey of Pennsylvania. But being chiefly sections of younger rocks than ours, the Austrian diagrams exhibit a more disturbed surface, so far as regards faults and slips, snapped anticlinals, upshoved synclinals, lapped folds, insertions or knife-edge intrusions of fragmentary strata, &c., while the main features are all the same. This may hereafter be adduced by some one as an evidence of the necessity of assigning quite a modern date to our contortions; inasmuch as disturbances, *relatively* as old (that is, occurring when the palæozoic rocks had as yet obtained no greater consistency and compactness than the newer secondaries or tertiaries of the Alps) ought to have dealt as hardly with those, as the Austrian subterranean forces have dealt with these. But that would be a hazardous conclusion; for the na-

ture of the bottom on which they lie probably determines, more than any other determining cause, the amount of disturbance in the normal curves of an uplift. The lateral thrust of horizontal tertiaries over a ragged bed of already upturned secondaries, or of flat and soft kainozoic strata over an already formed palæozoic topography, cannot result in symmetrical anticlinals and synclinals; and the amount of hitch and catch below, and therefore of crack and shove above, must be proportional, (1) to the roughness of the old surface, and (2) to the thinness of the new formation. But in the case of the Appalachians, both these proportionals are in the lowest ratio: (1) The palæozoic mass is seven miles thick, and (2) It lies conformably on the "azoic," if not on the "hypazoic" surfaces, so far as we can see, or with local exceptions; and there is reason to believe that where this is not the case geologically, it is the case practically; for the Potsdam sandstone, Quebec group, Taconic system or Primal Formation (whichever name we prefer), probably lies upon an already planed off surface of Laurentian primaries. Hence the wonderful symmetry of the palæozoic vaults and basins, the almost total absence of faults (until one goes far south), and the infrequency and smallness of earthquakes.

Hence also the high probability that the anticlinals were unbroken at the crest. A broken anticlinal must, in ninety-nine cases in a hundred, develop a fault. In the south, a system of broken anticlinals have developed a magnificent system of parallel faults. If the symmetry of our northern anticlinals is the *first* argument against their being originally broken, the absence of faults is a *second;* a *third* is to be found in the many instances of unbroken small anticlinals, unbroken even when overturned and collapsed; a *fourth* is to be found in the absence of any trace of a break in the symmetrical end mountains, formed by the closing of the outcrop walls of an anticlinal valley at both ends of it; a *fifth* is to be found in the side gap structure, which universally accompanies and characterizes the anticlinal structure; a *sixth* is to be found in the total absence of lakes along the anticlinal axis; a *seventh* is to be found in the evident compensation for room lost by room gained along any given cross section.

At this last point I think lies the solution of the problem. A true section of the crust, transverse to the waved structure, would show a perfect compensation between the sum of the outside and inside curves of the side by side lying anticlinals and synclinals; such a compensation as would distribute the slip between the rock faces, or back-and-belly planes of the stratification, through the whole mass, and thereby reduce it at any given point to a minimum. This dis-

tribution of the slickensides movement, taken into connection with the crumpling up into subanticlinals, and the tongue-shaped crimpling of the softer measures inside of these*, must have relieved the strain upon the outside of the synclinals below and anticlinals above, and set quite aside the necessity for those yawning gaps which are supposed by many to have occurred along all the great anticlinal axes of the region. It may be safely taken for granted, that had such occurred on the upper side of the anticlinals, similar ones would have occurred on the under sides of the synclinals, of which we see no trace. That the slipping of stratum upon stratum has gone on everywhere is everywhere evident. The softer formations have been most injured by it, and are penetrated by crumplings when the harder strata have splintered and fissured. But, as a whole, the plicating energy must have acted with a steady evenness of thrust, which carried up the anticlinal waves of the crust unbroken, and in some cases to a height of between five and ten miles above the present surface level.

By what agency could these masses have been removed, without leaving Alpine ranges, with serrated summits and protuberant spurs? Can we imagine the Pyrenees to be reduced by ordinary atmospheric erosion to the condition of the Jura? Giving even infinite time, will the desired result be ever attained? On the other hand, given a homogeneous element with sufficient force, acting either by one or by repeated blows, the result as we now see it on the present ground was demonstrably certain to come from the conditions which we see to have existed on the former ground. No one will deny that water, if obtained in sufficient quantity at a sufficient velocity, would be such an agent. In the acknowledged instability of the crust of the earth, and in its acknowledged less stability in ancient times than now, we find the possibility, nay, we feel the certainty, that the oceans have at times been launched across the continents, and we need nothing more to satisfy all the conditions for an explanation of Appalachian topography.

Parts of a private letter from Leo Lesquereux, Esq., of Columbus, Ohio, were read respecting the fossil botany of the coal, and the publication of manuscript memoirs in prepara-

* See Plate V for a few instances of this structure as yet unpublished. Many others, even more instructive, can be obtained in our various collieries. The western edge of the Broad Top basin is remarkable for the number, symmetry and regular sequence of these tongues; but they are common to all the anthracite basins. Fig. 5 is an accurate representation of one near Beaver Meadow.

tion to illustrate it; and also respecting the character of the Millstone Grit or Subcarboniferous Conglomerate in the Far West. Of the first he says:

If it is finished according to my original plan, it should have at least one hundred and fifty plates. There is no fossil flora of the coal; that is, there is no work on the subject, where one can find figured and described all the species of any coal-field. All has been made by fragments. Brogniart's fossil flora is not half finished, and will not be continued. Lindley and Hutton have published plants of all the palæozoic rocks of England, but all is mixed there, and no part is complete. Göppert has all his published fossil plants disseminated through a number of books, of which no one contains a complete series. Now, for the fossil flora of our coal-measures, I would like to publish drawings and descriptions of all the species, even if these species are already known and published from the coal of Europe; for a double purpose: firstly, in order to enable the student to proceed in the study of our fossil plants without the cost and incumbrance of a large library, which is now impossible; and secondly, to show from the beginning of the vegetation of our earth, the remarkable similitude of American with European types, always broken by characters of dissimilarity as difficult to appreciate now, as they were at the epoch of the coal. . . . It is an entirely American and original work. . . . You well know that everything has been, so to say, put in my hand for such a work. After the Pennsylvania survey, I have had those of Kentucky, of Arkansas, of Illinois, Indiana, Ohio; this last on my own cost. All the best collections of fossil plants of the United States have been sent to me for examination and classification, and thus I have seen an immense number of specimens, without counting those which I have collected myself. Would it not be wrong to abandon and lose the result of a work of so many years, and the advantage of so fine an opportunity for study, and leave the work unfinished, merely because I do not know how or when it can be published? I will go through if I can, or as long as I can, and if the work is good, it will come out in some way, even if I am not more of this world. . . .

I do not send you now the plates of the fossil plants of the Tertiary, ten in number. . . . All the Tertiary fossil plants that I have had under my examination, those of Mississippi, of Kentucky, and of Vancouver's Island, would make about fifteen plates. . . . Some questions of true scientific importance might

be discussed with their publication: 1st. The relation of the actual flora with that of the Tertiary. 2d. The comparative identity of typical forms both on the Pacific and Atlantic shores. By comparative identity I understand relation of the now living plants on both the Atlantic and the Pacific shore, with the fossil flora of the same country; the relation of the Vancouver Tertiary with the California flora, and the relation of the Tertiary of Mississippi, &c., with the Atlantic flora. Of course this does not indicate a relation of vegetation between both sides of the continent, either at the Tertiary epoch or now; on the contrary. 3d. The difference of flora of Europe and of America, at the epoch of the Tertiary, showing the separation of both continents. You know that Heer argues, on a supposed but unreal identity of typical forms at the Tertiary time, and concludes in favor of a Continental connection, either by an Atlantide, or something of this kind. 4th. The relation of forms of the Tertiary and Cretaceous floras, &c., &c. . . .

With this letter I send you two sections of the Arkansas conglomerate measures, and the underlying subcarboniferous measures. I am, indeed, very sorry that my sections were not made with more details and exact measurements; but I am not answerable for the deficiency. My assistant, Mr. Cox, had charge of the geological part of our explorations, and . . . we had for our measurements only an aneroid barometer, which, though pretty good, gave us only approximate altitudes.

The first general section, showing the true position of the coal in relation to the inferior strata, was taken fourteen miles southwest of Fayetteville, in Washington County, on the high waters of Middle Fork of White River.

	FEET.	INCHES.
1. Millstone grit in alternating beds of coarse, gritty sandstone, conglomerate hard sandstone with small pebbles, ferruginous hard bands, and soft shaly sandstone and shale, .	300	
2. Gray laminated shales passing at some places to ferruginous, very hard conglomerate. Shale band, . . .	2	
3. Coal, ...	1	6
4. Hard, black fire-clay full of stigmaria, passing at the base to clay-iron ore, ...	6	
5. Hard limestone with Encrinites, Terebratulæ, Archimedes, &c., &c. Upper Archimede limestone. It appears to be united with the lower bed sometimes, and then thickens to 20 feet, ...	10	
6. Blue soft shales with pebbles of carbonate of iron, . .	20	
7. Shaly sandstone (gray metal of the miners), . . .	30	

8. Hard coarse limestone, with great abundance of the same fossils as the upper one, ... 10
9. Cherty limestone, mostly hard silex, ... 6
10. Coarse sandstone with plants, Stigmaria, Sigillaria, Lepidodendron, &c.,* 60
11. Coarse limestone, lower Archimedes bed or middle bed? 30
12. Coarse and soft brown sandstone with a great number of fossil shells (Knob sandstone?) 100
13. Hard black limestones with fossils, ... 40
14. Black and yellow shales with carbonate of iron (Devonian?) 40

The second section is that of the Millstone Grit, and is taken from the base to the top of the Horsehead Mountain, a part of the Boston Mountain in Johnson County. It is,

FEET. INCHES.

1. Calcareous shales and argillous sandstone, containing a quantity of fossil shells. The top of the hill is covered, and apparently has a stratum of conglomerate; at least loose pieces of conglomerate are found above the exposed fossiliferous shaly sandstone. From top of hill down, . 30
2. Compact and shaly sandstones in alternating beds, . . 120
3. Massive, coarse, gritty sandstone, ... 80
4. Shaly sandstone, sometimes in banks, covered with vermicular impressions, and cut by hard bands (ferruginous shales), ... 120
5. Coarse, gritty sandstone, with conglomerate at the upper part. The pebbles are small, not larger than a bean, . 20
6. Hard, compact, gritty sandstone in banks, alternating with shales and shaly sandstone, ... 520
7. Red, yellow, and soft ferruginous shales and shaly sandstone, ... 150
8. Gray, hard, micaceous shales, mixed with pebbles of carbonate of iron, and having fossil plants especially near the base, ... from 10 to 70

(These shales at Horsehead have at some banks only six feet, at others as high as seventy feet. They pass to sandstone at the upper part.)

9. Black, soft, earthy shales, ... 1
10. Coal, ... 10 inches to 1 6
11. Fireclay, black, hard, full of stigmaria to level of the creeks.

* In Illinois, the sandstone has the same plants, and is overlaid by one bed only of the Archimedes limestone. In Indiana, where this sandstone is absent, it is replaced by a thin bed of coal just under the upper Archimedes.

Remarks concerning the nature and variety of the Millstone Grit of Arkansas, are noted many times in my diary. Thus, I read: Passing the hills or divide between the affluents of White River, I find the Millstone Grit of still more varied appearance. Sometimes it is a coarse, hard sandstone, a compound of fine grains of quartz, true millstone grit very hard, and in thick banks. These are separated or underlaid by soft, easily disintegrated, shaly sandstone, and thus they break in large massive banks or pieces thrown down along the slopes of the hills, or in the narrow valleys. Sometimes the same formation is mostly a compound of shaly sandstone, alternating with ferruginous shales, separated by thin beds of clay iron ore, or even of hard fireclay, without any trace of conglomerate. Sometimes the sandstone becomes black, ferruginous, and is here and there cut by a narrow *streak* of conglomerate, whose quartz pebbles are rarely larger than a small hazel-nut, generally much smaller. The Millstone Grit-measures are far more persistent in their thickness in Arkansas than in the East, and as the top of it has not been seen anywhere, the highest mountains being too low, it may be supposed that its thickness is greater than it has been measured at Horsehead Mountain.

Of course, though the variety of appearance and the great thickness of the Millstone Grit of Arkansas can be compared with some parts of the coal-measures of Nova Scotia, referred by geologists of that State to true coal-measures, we cannot conclude an identity of formations. From what you published in the Proceedings of the Society, I agree with you, and readily believe that the Nova Scotian basin is a separated member of our great American coal-fields. The flora of both the Canadian and the United States coal-fields is apparently the same. At least, of all the plants published by Bunbury, too few in number, indeed, to permit a satisfactory comparison, there is now no one that has not been found in our coal-fields. *Odontopteris cuneata*, Bunb., was for many years unknown in our coal-measures, but I have found it in plenty two years ago at Murphysboro', Illinois. This and a few other of Bunbury's species have not been found in Europe. But contrary to our opinion, we have these facts, that the anthracite basin of Pennsylvania is from all appearance the shores of a coal-basin. That Dawson finds in Canada an abundance of fossil coniferous wood; that the English naturalists assert that such wood is also in plenty in the coal-measures of England, while I can find none in ours; that also from Dana's assertion, the fauna of the coal-measures of Nova Scotia is rather related to that of England

than to ours. If it is so, we would have on our continent an anomaly of relation contradicted by what we know from the other formations. Good and long palæontological researches may help to settle the question. . . . .

It is certain that our coal-measures are increasing in thickness eastward, especially for the sandstone and the shale strata. Admitting, if not a continuity, at least a contemporaneity of formation under the same influences, and a continuation of increase of thickness in the same direction and the same proportion, this would already give us many hundred feet of difference for Canada. I understand moreover, that a shore formation of the coal has been necessarily subjected to a great many local variations, which could not reach an inland one. It is clear that the invasion of the sea, bearing with it sand and other materials, could not always penetrate the inland part of the basin, and cover the whole of it. This accounts for the multiplication of strata, and the dividing of coal strata into thin and numerous seams. Of course, if such divisions did happen on the shores, while the internal part of the basin continued in the same state of a continual peat growing, boggy marshes, the vegetation of these partial coal-seams cannot be variable. So local vegetation is always affected or directed by the general one, and the difference of vegetation of our coal strata becomes especially evident, after such cataclysms or such changes of level under the influence of which the whole extent of the coal-measures was covered with deposits, brought by water or formed under it, viz., sandstone or limestone.

It is therefore evident, that even if it was based on well ascertained facts, the sixth objection of Professor Dawson would be of little importance, especially in its application to our coal-measures. But I find it extraordinary to say the least, when compared to other assertions of the same author. In his last valuable paper on the flora of the Devonian period (Quarterly Journal, November, 1862), he says, page 328 : "Some species which appear early in the Devonian period, continue to its close without entering the Carboniferous; and the great majority of the species, even of the Upper Devonian, do not reappear in the Carboniferous period, but a few species extend from the Upper Devonian into the Lower Carboniferous, and thus establish a real passage from the earlier to the later flora. The connection thus established between the Upper Devonian and the Lower Carboniferous, is much less intimate than that which subsists between the latter and the true coal-measures. Another way of stating this is, that there is a constant gain in the number of genera and species,

from the Lower to the Upper Devonian, but that at the close of the Devonian, many species and some genera disappear. *In the Lower Carboniferous, the flora is again poor, though retaining some of the Devonian species, and it goes on increasing up to the period of the Middle coal-measures, and this by the addition of species quite distinct from those of the Devonian period.*" Is not this acknowledging a continual change in the vegetation of the coal epoch, from its beginning to its end? for we cannot admit of course that according to the views of Professor Dawson, such a change has taken place in the Lower Carboniferous, to stop at once at what he calls the Middle coal-measures. And can we not then conclude that with careful and long researches, at such places where the stratification is perfectly well fixed, these changes of vegetation may be recognized in strata of different horizon, and thus used for comparison at other localities?

The discussion concerning the *true Carboniferous measures* is, as you say, tedious and useless, at least when it is made without comparison with what we have around us. . . . . It is certain that the plants of the red shales (Vespertine or Ponent of Rogers), are different from those of the *coal-bearing* measures. There is, indeed, a gap in the vegetation between the *Red shales* and the Conglomerate, even near their horizontal line of union at Pottsville, while in the West, the true coal vegetation descends as low as the Upper Archimedes limestone, or even lower. This anomaly is to my persuasion merely apparent, or the result of causes without connection with the stratification. The vegetation of the coal must be always considered as a peculiar, and if I can say so, as a local one, born and continued under peculiar influences, and thus without a necessary specific connection with that of the open and dry land; a true peat or bog vegetation. I have many times taken the trouble to compare the vegetation of our peat bogs with that of the country at large, to show how the first one would be insufficient to give us even a slight idea of the last. Now the shales of the Old Red sandstone were evidently formed by open extensive flats, alternately covered with water or left dry, and thus, having a peculiar vegetation far different from that of the Bogs, which were always under the influence of a continual internal and external humidity. Compare, for example, the vegetation of the flats of Holland around Groningue, with that of the peat bogs of the same country, and seen just near the border of the same flats. There is not a single species common to both formations. The same can be said of our coast flats, and in New Jersey, you have the same peculiar difference of vegetation near the shores; the one covering

all the part that is reached by marine water, or rather by the tides; the others beginning in lagoons, where the water ceases to be subjected to alternate changes. Thus it has happened, to my persuasion at least, that there has been formed at some place red sandstone, with peculiar remains of vegetation and coal shales, though inferior in geological horizon with true coal plants. And for this reason, I say, that we have to admit the vegetation of the Old Red and that of the Coal, without putting too great reliance in the data furnished by palæontological botany concerning the age of stratification. But remark that I say this only of the red shales, compared with the coal formation; for indeed, as Professor Dawson says it, the slow change of vegetation in the coal-measures (putting aside the Old Red), is apparent from the lowest coal under the Archimedes limestone, to the highest strata of the coal-measures.

Pending nominations Nos. 481 to 491, and new nominations Nos. 492, 493 were read.

And the Society was adjourned.

Stated Meeting, April 3, 1863.

Present, eleven members.

Dr. Wood, President, in the Chair.

Letters accepting membership were received from F. Forchhammer, dated Copenhagen, March 11th, 1863, and from Max Müller, dated 64 High Street, Oxford, March 14th, 1863.

Letters acknowledging the receipt of publications were received from the Royal Society at Upsal, September 15th; the Royal Geographical Society at Vienna, October 15th; the Royal Geological Institute at Vienna, October 4th; the Society at Wiesbaden, November 1st; the Geological Society at London, January 7th; and the Antiquarian Society at Worcester, March 2d, 1863.